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THE STUDY OF ATMOSPHERIC CURRRENTS BY THE AID OF LARGE TELESCOPES, AND THE EFFECT OF SUCH CURRENTS ON THE QUALITY OF THE SEEING.

A. E. DOUGLASS.

THE writer's observations on this subject began at the Harvard College Observatory, in Arequipa, Peru, during the summer of 1892. That observatory is situated close to a river valley, down which, on clear nights, a swift stream of cold air descends. In the latter part of the night this frequently attains such a volume as to flow over the observatory grounds. During the summer mentioned, the thirteen-inch Clark refractor was devoted to observations on Mars, and it was noted that when this cold air reached the objective the seeing was immediately ruined. Frequently, an interval of some minutes could be perceived between its first entrance into the dome and its attaining the height of the objective. When the seeing on Mars thus became extremely bad, if the eye-piece was removed, and the eye placed in the focus, fine parallel lines could be seen to move swiftly across the illuminated lens in the direction of the wind, the lines themselves always being parallel to their motion. When this current once became established, no more good seeing could be expected for the remainder of the night.

After leaving Arequipa the subject was practically left in abeyance until the summer just passed, but in the preceding February some experiments were witnessed at the Clark manufactory which have a bearing on the present subject. The Yerkes forty-inch lens was at that time placed in position for the Foucault test. Mr. Lundine, Mr. Clark's head-workman, courteously explained the methods of using the apparatus, and

obligingly performed a few experiments to show how exquisitely delicate it is in detecting small differences in temperature. For instance, he placed a lighted lamp in front of the lens, and a dense vertical column of heated air could be seen rising from the chimney. A lighted match produced a violent commotion. Even the bare hand held in view was seen surrounded by dark lines, indicating higher temperature, and frequently small masses of air would get sufficiently warmed and ascend. The warming effect of a person standing some four feet from the optical axis could be observed without difficulty. At my suggestion he placed the lamp beneath the optical axis, and close to the eye-piece. This caused the lens to appear to move bodily. Finally, he waved rapidly to and fro a piece of light board to try the effect of motion in the air. It was with greatest difficulty that any increased pressure could be seen. As the board (seen edgewise from the focus) passed, for example, downwards, a very faint line curved upwards and away from its end, similar to the wave produced at the tip of an oar.* The idea of measuring cloud-heights by the change in position of the eye-piece to bring them into focus was broached, but received only discouragement from him.

The similarity between atmospheric currents, and the effects of temperature in the Foucault experiment was at once apparent. The fine parallel lines or waves, by which a current is detected, are unquestionably caused by variations in density of the air. This in turn may be due to rapid motion of the air, or to the contact of two bodies of air at unequal temperature, or to inequalities of temperature throughout the mass. The term "wave" as used in this paper refers to the system of (usually) straight parallel lines, placed at substantially equal intervals and of approximately equal densities, which is seen through an objective of sufficient size. We may define it more generally as the resultant of all unequal densities in a body of air, whether upon the upper or under surface of that body or extending throughout its mass. Whatever may be the actual form of this resultant, its motion past the objective is so rapid that it customarily appears as a series of parallel lines, moving longitudi-

* The writer deeply regrets that lack of time prevented the systematic pursuit of these experiments. They are described from memory.

nally as described. The analogy to waves on water is shown in the resemblance between shadow bands in a total eclipse, and the light markings on the bottom beneath water disturbed by the wind.

These experiments tended to show that inequalities of density in the air are more likely due directly to its temperature changes than to its rapid "gust" motion. But this was stated provisionally because no quantitative experiments were made, and it still remains in uncertainty.

Again, when the lamp was placed near the focus, the air waves became very large in comparison with the bundle of rays which eventually entered the eye. Instead of producing so much confusion over portions of the lens, the entire lens moved bodily. This suggested the possibility that bad seeing might sometimes be improved by stopping down the lens, thus making it more nearly the same size as the air waves, and producing a large bodily motion in place of a complete confusion of fine planetary detail. The loss in light and contrast is the serious objection to this procedure, but the matter deserves further experiment by means of large telescopes and under different grades of seeing.*

One other inference was drawn at the time from these experiments, namely: that the slightest change of temperature in the dome or in the telescope tube, would be harmful. However, even in this comparatively steady air the effect of lanterns near the telescope cannot be perceived at all. It will be an easy matter to try heating the dome, or some part of it, and by simply looking through the objective at some planet to tell at once if harm is being done. On some extremely good nights the

* Prof. W. H. Pickering has devised a scale of seeing which is simple and accurate, though it varies for different apertures. It is as follows: —

With sufficient power (100 to 150 to the inch) the star image consists of a large central disk and a series of rings.

Seeing 10. Disk well defined, rings motionless.

Seeing 8. Disk well defined, rings complete but moving.

Seeing 6. Disk well defined, rings broken into dots and lines, but still traceable.

Seeing 4. Disk well defined, no evidence of rings.

Seeing 2. Disk and rings in one confused mass, constant motion, no increase in size.

Seeing 0. Disk and rings in one confused mass, violent motion, image greatly enlarged (for example to twice the diameter of outer ring).

effect might be bad, while on poor nights it might give the observer comfort without altering the seeing in the slightest. A slight examination of the ordinary atmosphere through a good lens will convince the observer that the Foucault test is immensely more severe than any test on a star ; that usually our lenses are much too good for their atmospheres.

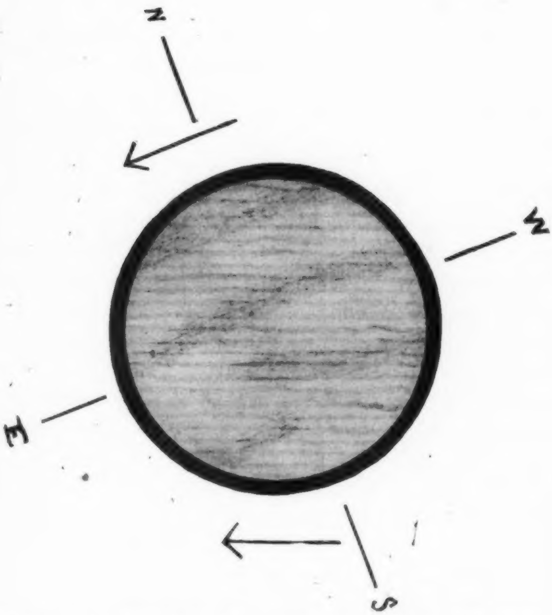
After this introduction we may take up the remainder of the subject under special headings.

I. Time and Development of Recent Observation.

The first observation made at this observatory was on Sept. 28. By placing the eye in the focus of the eighteen-inch Brashear telescope when turned on Mars, a swift north-east and a slow north or north-west current were found. The direction of the currents was obtained by considering the waves to be arcs of great circles whose intersection with the horizon gave the required azimuth. This same method is yet used, but while it answers for present purposes, more accurate determinations should be made. On the following night the size of the wave was described by stating a rough estimate of the number of waves, side by side, within the diameter of the objective. Yet this estimate was so very rough that it has often seemed sufficient for the most part to merely state that they were fine, medium, or coarse. During the following week the changeableness of each wave system, and its conspicuousness, began to be noted, though the importance of each separate item for completing the observation was not at once recognized. Readings of the thermometer were taken with fair regularity as bearing on the subject. Omissions have been supplied from the daily readings of the minimum thermometer.

Shortly after the middle of November, attempts were made to estimate the swiftness of the currents, but it was not until Dec. 2 that a direct effort was made to measure their height. This at once disclosed another vitally important feature, namely, their relative heights, for, in viewing them through the objective alone, they are projected one upon the other, and in nearly every case it is impossible to tell which is lowest.

Special observations have been made at various times. The effect of using different celestial sources of light was tried on November 20. The effect of different sized telescopes had



ATMOSPHERIC CURRENTS SEEN THROUGH AN 18-in. LENS, DEC. 24, 1894, AT FLAGSTAFF, ARIZONA. SCALE, $\frac{1}{10}$.

The fine lines belong to the higher current, whose lower surface was observed to vary in the course of observation from 3,400 to 5,400 feet above the ground, (10,600 to 12.600 feet above sea-level.) The waves were on the average 0.8 inches wide, and moved with a velocity estimated at 17 miles per hour. The coarse lines belong to a lower current which was felt as a strong west wind. The waves had a mean width of 6.0 inches, and moved with a speed estimated at 8 miles per hour.

been tested a week or two before that date. The effect of passing clouds was noticed at various times, especially on November 27 and 30 and December 16. On many other dates some record was made of the general weather.

II. Different Forms of Waves and Method of Observing and Describing Them.

While all wave systems are sufficiently alike to be recognized as essentially the same thing, there are three forms that present individual features.

The first form, recognized from the very start, consists of the straight, equal parallel lines, equally separated, moving longitudinally. This is the typical form. They are not always of precisely equal density, nor are they invariably separated by equal intervals. The second form, though noticed before, was not regularly recorded until early in November. The lines in it are extremely fine and occasionally parallel, but all twisted and bent, and move slowly or float in any direction. A slight predominance of movement in the direction of some wave system indicates that they are not currents inside the telescope tube or within the dome. They were, from an early date, supposed to form the stratum lying next the earth, and observation with the eyepiece has confirmed this view.

The third form is not yet understood. It partakes of the character of each of the others. The lines are usually very fine, parallel, and straight, and the longitudinal motion very rapid, yet they appear irregularly, and over very limited areas, producing the appearance of bands or ribbons crossing the objective. This may be called the ribbon form, as the preceding has been named the floating or syrup form. The actual elevation of the ribbon form has not yet been noted; in fact, it is of rare occurrence and was first separately recorded on December 17.

A certain combination of currents of the first form produces such a perplexing appearance that it deserves a word in explanation. It has been termed a "vibrating" appearance. The objective appears strewn with large regular spots which pulsate. No movement can be assigned because no current is visible. The probable explanation is that two wave systems of equal size are crossing each other at such relative velocities and direction as to produce the "obstruction" figures noticed when one

looks through two pieces of wire gauze, whose wires lie in the same plane but differ in direction. The vibrating effect might be called "twinkling" from its appearance, and it is possible that to the unaided eye it increases, or perhaps is the chief cause of, that effect on stars. Sometimes a single current will have a "fluttering" appearance, somewhat similar to the "vibration" and probably produced by a second current much fainter than, but quite similar to, the first.

Enough has been said to suggest the method of observing and describing atmospheric currents; but a brief review will bring it into a more compact form.

The definition obtained without the eye-piece is far superior to that with it; the observer begins, therefore, by placing the eye in the focus of the objective, and noting down the direction, steadiness, fineness, swiftness, and conspicuousness of each current, distinguishing them by letters of the alphabet, and making a special note if they belong to the second or third class. The eye-piece is then replaced, and brought to a focus on the micrometer threads. A scale on the side of the draw-tube reads zero when the planet (Mars has been used almost exclusively for this work) is in focus on these threads. Then the eye-piece is slowly turned out. Scale-readings are made as fast as currents appear or change from one to another, and at each reading estimates of the velocity of the current in view are made. After the eye-piece is carried out as far as the draw-tube allows, the process is reversed and check-readings taken on turning it in. Recent observations have included the altitude of the star or planet, and the amount of motion (due to the atmosphere) it exhibits when examined in the ordinary way.

III. Effect of Using Different Sources of Light.

This may be very briefly stated. The larger the source of light, the less is the contrast between the waves and the valleys, if they may be so named. The twinkling of a sufficiently small source of light appears to be due to the concentration or scattering by these waves of the pencil of light, which enters the eye; the farther apart the waves, the larger may be the source of the twinkling light. In a large telescope a star does not twinkle, because the objective includes several waves and valleys. Stars that show no variation in brightness in a six-

inch telescope may twinkle decidedly in a two-inch finder. Yet, this is not the whole explanation, because stars sometimes twinkle when the atmospheric waves are very inconspicuous. It may be due to a wave so coarse that the telescope does not show it.

IV. Effect of Different Apertures on Currents and Seeing.

At the present writing, the original observations on this subject are not at hand, but theory and observation agree. The size of the waves does not vary with different apertures, nor, in fact, do they differ in any respect save that with a small glass, it is more difficult to study them.

The effect on seeing is what we should expect. In the smaller instrument, when the width of the wave is nearly as large as the diameter of the objective, there is more bodily motion, and in the larger, a steady limb with confusion of detail. In fact, we have, in work on Mars, frequently expressed the quality of the seeing by two figures, — one referring to the amount of detail visible, and the other to the steadiness of the planet.

The present topic leads us to the question as to whether stopping down the lens can ever improve the seeing. Let us suppose that the maximum refraction produced on each slope of a wave amounts to $\frac{1}{2}''$. There the total vibratory effect on each point of the disk of the planet will amount to one inch, and if the objective includes several waves, we shall have a steady but ill-defined limb. The air waves vary more or less from one-fourth inch to six inches in distance between crests, with a customary distance of one and one-half inches. Suppose, for example, that the waves are four inches apart. It is evident that if we stop the lens down to two inches, we shall see the entire planet vibrate through $1''$, at a rate depending chiefly on the speed of the current, but there will be no confusion of detail, unless the current be travelling too fast for the eye to follow this motion (which I am inclined to consider unusual). The loss of light, however, will be enormous. If we use a four-inch stop, the planet will alternately show confusion of detail and misplacement.* Six inches will still give us mis-

* Sometimes I have seen Mars rapidly change its outline from a circle to ellipses of varying directions and eccentricities, due probably to this relation of air wave and lens.

placement, but the loss of detail will be more pronounced; and the more we add to the aperture, the more certain is the loss of all detail, smaller than 1″, and the steadier appears the ill-defined limb. Experience declares that a steady ill-defined limb is better for micrometer work than a moving limb of perfect outline; for such work, therefore, a full aperture evidently is preferable. But confusion in planetary detail is lessened by greatly diminishing the aperture, and if the loss of light be not too great, advantage may result. Two influences in practice interfere with these conclusions, namely, cross currents of different sizes, and the changeableness of the very current we may be trying to improve.

V. *Effect of Different Currents or Their Combinations on Seeing.*

Since a perfect lens is one which, in the Foucault test, shows no irregularities of illumination, it is evident that bad seeing, if not the worst, must exist where the greatest atmospheric disturbance is revealed by the objective. This is rarely caused by a single current. Even a conspicuous current, if alone, may give seeing four on the scale mentioned. The addition of a second current nearly always reduces it to 1 or 2. If the added current be of the second form, seeing 0 to 1 results; the effect of the third form has not been observed.

Almost invariably, two currents of the first form are visible, and curiously enough, they nearly always form an angle of 45° or less with each other. When it is more than 45°, the waves are likely to differ greatly in size. Usually, one of these is continuous and the other variable, changing its direction occasionally and frequently ceasing altogether for a time. This is the cause of the variation in seeing from moment to moment. Frequently, one or two currents of form I exist with seeing from 2 to 5. Subsequently, if the syrup form appears rather conspicuously, the seeing drops to 0. This can be explained by assuming that the refractive power of the syrup form is greater than that of the others. That may be a result of its lower elevation. If the waves exist only at the surfaces of contact between bodies of air, their refractive or "confusing" power must depend largely, if not wholly, on the difference of density between such bodies. But atmospheric refraction depends on the same thing. It may be that some quantitative

connection will be discovered between the two, though at present this involves an assumption which we are by no means prepared to maintain.

Two currents do not necessarily mean bad seeing. If they are very faint the seeing may reach 6 or 8, but even then the best moments are when one current disappears and but a single faint one remains.

VI. Effect of Clouds on Currents and Seeing and the Cause of Coarse and Fine Currents.

It has been stated that detail may sometimes be improved and the limb made less steady by diminishing the aperture. The same result may be obtained from increase in the coarseness of the air waves. This is one reason why the seeing sometimes is very good amidst clouds. When clouds are passing the waves of the most conspicuous current are very apt to change rapidly in size. They are usually fine before the cloud comes on, they may be fine or coarse while it is passing, and are more likely to be coarse immediately after it has gone. The effect on the planet is what we should expect, — steady and ill defined before the clouds come on (detail 3, limb 7), usually steady and very badly defined amidst them (detail 2, limb 10), and well defined but in considerable motion after they pass (detail 7, limb 4).* This observation has been repeated several times on clouds moving past the telescope, but always, it so happpens, on that kind of cloud, which, by its tendency to dissipate and reappear, shows the air to be full of moisture.

This fact, that air waves change their size in the presence of clouds, suggests that temperature influences their size, and that they grow coarser with its increase. But we get little more than the suggestion, for evidently the higher currents must be the colder and yet they have been coarser in at least three cases out of five. Density may have something to do with it, but rapidity of motion does not, apparently, because they do not change their velocity in the vicinity of clouds, nor in general do the coarse currents differ conspicuously in speed from the fine

* These figures represent, on a scale of 10, the perfection of the detail and steadiness or freedom from bodily motion of planet. They are made to agree as far as possible with the stellar standard of seeing.

ones. Direction has no influence. A more plausible hypothesis is that their size depends on freedom from moisture. This accounts for the upper ones being usually coarser and is not incompatible with the observed effect of condensation. Condensation of moisture produces a slight temporary warming and consequently drying of the surrounding air. The cold and relatively moist air that descended the river valley at Arequipa was (if correctly remembered) composed of very fine waves, and certainly the syrup form of current almost universally presents an exceedingly fine structure.

VII. Direction of Upper Currents.

The majority of records show a general westerly current, but up to the present writing so few observations on relative heights have been made, that no generalization can be formed. Out of eight actual observations the highest current came from between S. W. and W. N. W. five times. Once it came from the north, once from the south, and once from the east. In two cases of westerly upper wind, no lower current was visible; of the remaining six, three times the lower wind differed from the upper by two points of the compass, — once by four points, once by six points, and once by a quadrant.

VIII. Height of Currents and Clouds.

The means of getting the height has already been mentioned. With the focal length of the telescope, F, and the extension, *e*, of this focus, in order to bring an object into best definition, the distance D of that object is thus found: $D = \frac{F(e+F)}{e}$ It is easy to construct a curve which will give D for different values of *e*. The height of the object equals D into the cosine of the zenith distance. It is evident that the longer focus and larger aperture a telescope has, the more accurately will it give these distances. Not the least puzzling part of it is why these currents should come into focus at certain given positions of the eye-piece. The rays of light from a star, for instance, being parallel, it is easy to see how interposed air waves become visible in unchanged dimensions when the eye is placed in the focus of the lens. Similarly it would seem more probable that they should show equally good definition through a great range

of movement of the eye-piece, but such is certainly not the case. The explanation seems to be that they diffuse much more light than we expect. It is not impossible that while the heights obtained represent something definite, they may have been influenced by the refractive power of individual waves.

Much yet remains to be learned in regard to the heights of atmospheric currents, but one point of some interest was brought out very clearly. On Dec. 23 and 24, during a rather strong west wind, it appeared that below a certain level the movement of the wind became intermittent, that is, it moved in gusts. When the telescope was focused on some part of this disputed region, for a time currents would appear precisely similar to the steady stream above. Then an interval followed in which nothing but the syrup form showed itself, moving in any direction, only to be effaced by a second gust. The altitude of the lower surface of the steady flow of air was found to be about 3,400 feet on the first night, and between 2,900 and 3,500 on the second (altitude of observatory 7,250).

This conception of gusts reaching down here and there from an overhead current is not new. In Winslow, Arizona, early in May last, I assumed this to be the explanation of the sand-storms for which that place is famous. Standing in a slightly elevated position west of the town a broad expanse of desert was presented to view. As many as fifteen sand-whirls were seen at one time.* The sand-storm proper was caused by an extremely local wind. Judging the wind by the dust it raised, the average storm (or gust) of that day covered an area half a mile long by one quarter of a mile wide, and might move, in all, two miles with a rate estimated at ten miles an hour. They were scarce, there being, at a guess, one in ten square miles. The velocity of wind in the storm seemed to be much greater than the speed of the storm. While the storm was yet at a distance, the air was perfectly quiet; as it passed near by the motion of the surrounding air towards it could be perceived. All the while clouds were moving regularly by overhead.

The trouble with this method of determining distances is the difficulty of getting a good focus. Even the principal focus

* One of them deserves mention. It was a cylinder about two hundred feet in diameter. After travelling a quarter of a mile, it broke up into six small whirls which kept on an equal distance.

seems to vary with the character of the object. This needs to be studied. It was thought that cloud-heights might be easily observed, but in a trial on some cirrus cloud no satisfactory focus could be found. Cumulus clouds against a clear sky might give better results; and recently a trial on rather low clouds passing in front of the moon seemed to give sufficiently definite figures which agreed with estimates derived from the position of the cloud on a neighboring mountain. That the method can be successful if given sufficient contrast and definition is shown by the result obtained on measuring a known distance of nine and one quarter miles. The mean of six settings gave eight and one half miles (using the principal focus given by a star). This discrepancy is not surprising, when one considers that the maximum base-line used was only eighteen inches.

IX. Velocity of Currents and Clouds.

Velocities may be estimated with or without the eye-piece. There is no great choice between the two methods. Theoretically, using the eye-piece on a planet of large diameter should give the best results, for thus the movement may be followed through a greater arc. The difficulty with each method is the great speed of the current in comparison with the size of the field.

Without the eye-piece, we are evidently watching the unchanged motion of the air through a distance equal to the diameter of the objective. Sometimes the motion is slow enough to enable the observer to estimate the fraction of a second required in passing the objective, but usually it is so rapid that this method gives but the roughest possible approximation. A better plan, still exceedingly rough, is to give to the eye a movement equal to that of the current, and lasting one second or one-half second. This, repeated many times, will give at least an estimate of the number of diameters traversed in a given time.

With the eye-piece, the size of the field has to be computed from the position of the eye-piece and the diameter of the source of light. With a star, it remains constantly the same size as the objective, but with a planet this must be increased by the apparent diameter of the planet at the distance which happens to be in focus. The number of fields traversed by the stream of air

in a given time may be estimated in the same manner as before.

As to the actual velocities estimated in currents of Form I., they range usually between five and twenty miles an hour, and sometimes reach higher. This seems less than expected, and may partly be attributed to the uncertainty of the method, and partly to the fact that high velocities of surface winds are almost unknown in Arizona. The average hourly wind-velocity for the year is between six and seven miles.*

X. Effect of Currents on Local Winds and Weather.

This subject has received very little attention, but is included because it is deemed well worth a careful study. In two cases a verbal prediction was made as to the probable wind on the following day, and each was verified. As a rule, it seems a matter of chance whether the upper currents change their direction at night or day; if at night, the local winds will probably do the same on the succeeding day.

Nothing new has been learned in regard to the effect of currents on the formation of storms. We knew long since that summer storms came on southeast and winter storms on southwest winds. If it is true that fine-air waves indicate high relative humidity, combining that with the height and direction of currents may give us valuable assistance in predicting weather.

XI. Instruments for observing Atmospheric Currents.

For direction and size, two parallel wires may pass in front of the object glass or mirror, as the case may be. By rotating these from the eye-end and reading off a scale, the direction may be obtained. It could easiiy be arranged so that the direction could be read directly from a compass. One of these wires should be movable, and, by a scale showing its position, the size of the waves is given directly. For velocities, the largest possible aperture (at least in the direction of the current) is needed. From the focus, this should appear large enough to allow the eye to appreciate the rate of the current,

* See letter from the Secretary of War, on Irrigation and Water Storage, 1891. These observations, however, are all at lower elevations than Flagstaff.

and yet so small that the eye does not have to move through too great an arc in the given interval of time. If a long scale, on which to measure this motion, could be rendered visible by a prism close to the eye, it would greatly help. In this observation good definition is superfluous. In measuring heights, the larger the aperture, the better; obviously, because the aperture gives the base-line. The focal length probably makes little difference. Better definition is required here than for velocities; but it is quite probable that a large lens, cast and roughly ground — possibly a single glass — would answer the purpose for both these observations. Serious imperfections in the glass would not affect velocities, and in observing heights they could be covered with patches. If it was found easier in manufacture, a narrow section of a lens might be cast, giving full diameter in one direction only. Placed in the direction of currents, it would give their speed and direction, and placed transversely it would give size and height.

The remarks on this subject are merely suggestions. Doubtless if it is desirable to make extensive observations some suitable apparatus will be prepared.

XII. List of Observations from Sept. 28 *to Dec.* 31, 1894.

The time given in the following table is that of the 105th meridian. The observations are of such diverse character that the following abbreviations are necessary. Each current is, in the complete observation, followed by four characters describing it. The first of these refers to the continuousness of the current, and has one of four letters: *s* means steady; *i*, intermittent; *r*, rare; and *c*, changeable in direction. For the second letter, describing size, *f*, means fine; *m*, medium; and *c*, coarse. When the size of the wave is estimated, it is given in inches and tenths. The third letter gives the speed: *f* means fast or swift; *m*, medium; and *s*, slow. If the speed was observed it is given in miles per hour. The fourth letter indicates intensity or conspicuousness of the current: *f* means faint; *m*, medium; and *c*, conspicuous. Between two currents a space is left for the observed height of the division. A column is reserved for the third current which usually is of the floating form, so that Fl in that place signifies this form, and R, the ribbon form; small *r* means rare, and *f*, faint. The next column

gives the "confusion" or movement which the air currents cause in a body under scrutiny. This is really an attempt to give quantitative seeing. It is expressed in tenths of one second of arc, the first number referring to confusion of detail and the second to extent of bodily motion. The wind unfortunately was rarely recorded, but a column is reserved for it. In the same column, *h* means haze, and *c*, clouds. The temperature at the time of observation is given for the most part, but when failures were made to note it down, its place is supplied by the reading of the minimum thermometer for that night, preceded by the letter *m*. The final column gives the seeing at the time of observation. The figures average quite low, since these observations were chiefly made when it had become too poor for anything else. They also include an almost continuous month of bad weather, December, due, presumably, to the temperate zone circulation reaching down and including us in its moist currents.

Only the most complete observation for each evening is given.

September. d.	h.	I.	Ht.	II.	Ht.	III.	Conf.	Wind.	Temp.	Seeing.
28.	12.8	NE. —. —. f. —	....	NNW. —. —. s. —	....	....	....	NW.	33.7°	0 to 1
29.	14.0	S. —. 1·5. f. —	....	....	....	....	....	....	44.5	1
30.	14.1	W. — 0·4. f. —	....	....	....	....	....	....	32.8	0 to 1
October.										
1.	11.0	....	....	....	....	....	....	....	39.5	2 to 5
2.	11.1	ENE. —. 1·0. f. —	....	N. —. 3·6. m. —	....	....	....	....	46.5	1
3.	14.3	E. s. 0·9. —. —.	....	NE. c. 0·9. —. —.	....	....	....	....	49.8	1 to 2
4.	14.9	NE. —. 0·9. f. f.	....	....	....	....	....	....	54.6	5 to 9
5.	15.0	SW. — 0·6. f. f.	....	....	....	....	....	....	m46.0	3
6.	—	....	....	....	....	....	....	....	m44.9	2 to 8
7.	14.6	NW. —. 0·9. f. m.	....	SW. i. 3·0. s. —	....	....	....	....	m46.3	3
8.	10.4	....	....	....	....	....	....	....	54.2	3 to 5
9–10.		No observations.								
11.	14.5	NE. —. c. s. f.							m47.	3 to 4
12.	14.2	NE. s. f. f. —	....	ESE. st. f. f. —	....	SW. or Fl.	....	....	m46.5	3 to 4
13–14.		No observations.								
15.	12.0	NE. — f. f. m.	....	....	....	....	....	....	m45.8	2
16.		No observations.								
17.	14.6	SW. — c. f. c.	....	WSW. —. f. f. c.	....	....	....	W.S.W.	m47.0	4
18.		No observations.								
19.	15.7	WSW. — f. f. c.	....	....	....	....	....	W.	36.9	3
20–22.		No observations.								
23.	14.6	SW. —. c. f. c.	....	NW. —. f. m. m.	....	....	....	W.	m45.8	2 or 3
24–30.		No observations.								
31.	13.3	WNW. —. f. f. c.	....	....	....	....	....	....	m38.1	0
November.										
1.	10.4[1]	NE.[1] —. c. f. c	....	....	....	Fl.		W.	m32.5	0
2.	13.9	W. —. f. m. m.	....	....	....	....	....	N.W.	m32.4	{ 8 to 10 for limb { 2 for detail.
3.	12.5	S. s. m. m. c.	....	E. i. m. m. c.	....	....	....		m40.9	{ 8 to 2 for limb. { 2 for detail.
4.	13.4	E. by N. — c. s. c.	....	E. —. f. m. f.	....	....	....		m40.8	{ 8 for limb.
5.	12.5	N. —. c. s. f.							m39.8	{ 2 for detail.
6.	13.6	NNE. —. m. f. c.	....	....	....	Fl.	....	W. by S.	m39.4	{ 4 for limb. { 2 for detail.

6.	13.6	NNE. —. m. f. c.	....	....	....	Fl.	....	W. by S.	m39.4	{ 2 for detail.
7.	12.9	N by E. —. m. f. c.	....	N to NW. c. f. —. c.	....	Fl.[2]	....	N.	44.0	1
8.		No observations.	....	....	....	....	....	....		
9.	14.2	NE. s. —. s. c.	....	E. i. —. s. c.	....	....	....	....	m38.5	{ 10 for limb. 1 for detail.
10.	12.0	E. s. m. m. c.	....	NE. to SE. i. m. m. c.	....	Fl.	....	....	m38.5	1
11.	9.5[3]	W. —. —. f. —	....	....	....	Fl. r.	....	....	m39.2	1 to 2
12.		No observations.	....	....	....	....	....	....		
13.	12.8	E. —. —. f. c.	....	II. is finer than I. and makes an angle of 5° with it.	....	....	....	....	m36.6	2 to 4
14.	13.4	W. by N. — f. f. f.	....	....	....	....	....	....	m41.6	4 to 7
15.	12.8[4]	SW. s. c. —. —.	....	W. i. f. —. —.	....	....	....	SW.	m37.	4
16.		No observations.	....	....	....	....	....	....		
17.	16.8[5]	NW. —. —. s. —	....	....	....	Fl.	....	....	m34.2	0
18.	12.4[6]	E. by S. s. f. f. c.	....	NE. i. c. m. c.	....	....	....	....	34.0	1
19.	11.7	NW. s. f. f. c.	....	....	....	Fl.	....	h. W.	38.0	2 to 4
20.	11.4	N. s. c. 10. m.	....	WNW. c. f. 10. m.	....	Fl.	....	....	m37.6	2
21.	11.0	SW. s. m. f. m.	....	W. r. f. f. f.	....	Fl.	....	....	39.5	3 to 4
22.	13.0	SW. s. c. m. c.	....	WNW. i. m. m. c.	....	Fl.	....	....	33.2	3
23.	11.9	SW. s. c. —. m.	....	NW. r. f. —. m.	....	Fl.	....	....	37.4	4 to 7
24.	12.6	ENE. s. f. f. c.	....	NE to ESE. i. f. f. c.	....	Fl. r.	....	E.	43.0	1 to 5
25.	8.2	S. by W. s. c. f. m.	....	W. i. f. f. m.	....	Fl. r.	....	....	m39.2	3
26.		....	....	....	....	....	....	....	m39.1	5
27.	7.0	SW. s. f. f. m.	....	W. i. f. f. f.	....	....	....	....	46.0	3
28.	6.9	WSW. s. f. f. m.	....	SSW. r. m. f. f.	....	Fl. r.	....	....	m36.0	4
29.		....	....	....	....	....	....	....	m30.3	0
30.	7.8	SW. s. f. to c. f. c.	....	W. i. f. f. m.	....	....	....	c. WSW.	39.0	1 to 2
December.										
1.	9.8	E. by S. s. 1·8. m. m.	....	NE. s. f. m. —	....	Fl.	....	....	29.8	2
2.	7.9	S. s. 1·2.m. c.	3,600 feet.	E. s. 1·2. m. c.	....	....	....	E.	33.0	1 to 2
3.		....	....	....	....	R. and Fl.	....	....	36.0	7
4 to 9.		Cloudy.								
10.	7.4	N. by W. s. 6. o. 19[1] c.	4,000 feet.	NE. by N. s. f. 19[1]. c.	3,000 feet.	Fl. r.	....	....	23.5	0 to 1
11.	7.2	SW. —. —. —. m.	....	W. —. —. —. m.	....	Fl.	....	c { H 3500 V 8	27.5	3
12.	7.6	S. by E. i. 2·6. 12[4]. m.	8,400 feet.[2]	ESE. i. f. 49.[5] c.	4,100 feet.	E.[3] s.f.f.c.	....	E.	30.8	0 to 4
13 to 15.		Cloudy.								
16.	8.2	W. by S. s. 1·5. f. m.	variable.	N. by W. i. 1·0. f. m.	2,100 feet.	Fl. r. f.	....	W. by N.[6]	29.5	1 to 2
17.	10.6	SW. r. f. 15. m.	18,000 feet.[7]	SSW. s. 0·6. f. c.	7,700 feet.	R.[8]	....	....	32.0	2 to 6
18 and 19.		Cloudy.								

September.	I.	H T.	II.	H. T.	III.	Conf.	Wind.	Temp.	Seeing.
d. h.									
20. 6.6	SW. s. 2·2. 8.[8] c.	3,800 feet.	....	....	Fl.[9]	....	h. c.	37.0	2
21 and 22.	Cloudy.								
23. 7.4	WSW. s. 2·2. 21.[10] c.	3,600 feet.	WSW. gusts to earth.	....	Fl. r. f.	2.6	h. c.	35.0	0 to 1
24. 8.5	WNW.[11] i. 0·8.14. c.	4,400 feet.[12]	W. s. 6·0. 7. c.	....	....	4.9	W.	29.0	0 to 2
25 to 31.	Cloudy.								

NOTE. All observations in this table were made by the writer, save that of Dec. 3, which was made by Prof. W. H. Pickering.

NOTES. Nov.

[1] Two observations were made that night: 10.4h. I. N. by E. — Lowell, observer and 18.7 I. E. III. Fl. Pickering, observer. This is the first recognition of the floating form. It undoubtedly had existed many times before.

[2] This Fl. was recorded at 17.4 h. by Prof. W. H. Pickering; he observed a NE. current in place of the N. by E. given.

[3] Observed by Mr. Lowell.

[4] A less complete but similar observation was made by Mr. Lowell at 7.3 h.

[5] Observed by Prof Pickering. This NW. current showed a "vibrating" effect.

[6] Observations verified in 6 in. and 12 in. telescopes.

NOTES. Dec.

[1] ± 8.

[2] Reading 20 m. later gave 18,200. Velocities taken at second reading.

[3] May be same as Current II.

[4] ± 4.

[5] ± 33. Too swift to estimate well.

[6] Currents I. and II. were very variable; sometimes together and sometimes one alone seemed to occupy the entire atmosphere. Clouds followed.

[7] At 9.0 h. this division was about 4,700 feet. Current III. was then of the syrup form.

[8] Taken with eye-piece. Without it a velocity of 14 was estimated.

[9] Average speed 2m. an hour from S.

[10] An estimate in which clouds interfered gave 14.

[11] This observation on Aldebaran. Previous observations on Mars showed more than usual variability in currents as to height, direction, and size.

[12] ± 1,000 feet. Varied between sittings.

In conclusion we should be reminded that the preceding observations are distinctly local, and that they cover a very brief portion of the year. Situated, as this observatory is, near the meteorological line which separates the temperate from the torrid zone, it belongs to neither one, pure and simple, but partakes of both; and perhaps especially complex is the mixture at this season of the year when winter is coming on and the southern limit of the temperate zone is moving equator-ward. But we may reasonably hope that future observations will render our knowledge more general, both as regards time and place.

LOWELL OBSERVATORY,
FLAGSTAFF, ARIZONA, JAN. 1, 1895.

THE STUDY OF ATMOSPHERIC ELECTRICITY AT SEA.

PROF. W. F. MAGIE.

DURING the summer of 1889, while at sea in the United States Fish Commission Schooner "Grampus," I had an opportunity to make a few observations on the electricity of the atmosphere, in the way recommended by Exner. My report was published in connection with Prof. Libbey's account of the expedition in the Bulletin of the Fish Commission, Vol. IX., but it has been suggested that a statement of my conclusions might still be acceptable in this JOURNAL.

The observations were made to determine whether the conditions at sea are favorable for systematic work on atmospheric electricity. It is difficult on land to find a place where observations can be made, which is free from the effects of irregularities in the earth's surface or the proximity of surrounding buildings, and it is impossible to avoid the presence of dust in the air in amounts which vary from hour to hour, and which introduce errors that cannot be estimated or allowed for. To such disturbing influences, Exner ascribes the occasional observation of negative potentials in the air, which, on his theory, are otherwise impossible. He especially states that, owing to the dust, observations made in the summer time are utterly unreliable. So far as my observations went, they showed that such disturb-

ances were not important at sea. A position can be selected on the vessel so situated that the slight changes in the vessel's trim, incident to the ordinary conditions of sailing, will have practically no effect at the points where the collector is held; and the constant effect due to the vessel as a whole can be determined once for all by simultaneous observations made on the vessel and in a small boat at some distance from it. In this way, the potentials relative to the true earth's surface can be obtained with more accuracy than under the usual conditions on land. Besides, the air above the ocean at long distances from shore is only in rare instances laden with dust particles. Flying spray might give trouble in rough weather, but in the pleasant summer days on which my observations were made there were no indications of any disturbances from this cause.

The instrument used was a Thomson's portable electrometer. A comparison of this instrument with Exner's portable gold-leaf electrometer led me to adopt it as more sensitive and more accurate. It is only fair to add that it is also much more expensive. The collector used was made on Exner's pattern, in which advantage is taken of the fact that a flame assumes the potential of the surrounding air. A candle, shielded by a sheet-iron chimney, was mounted on the end of a vulcanite rod, and a wire, the point of which terminated in the candle flame, led to the electrometer. I have since found that a more convenient collector can be made by substituting for the candle an alcohol lamp, in which the flame is formed over wire gauze. The flame of such a lamp is maintained in winds in which it is impossible to keep a candle lighted.

The potentials were measured at points two, three, and four metres above the deck of the vessel, on the weather quarter. In connection with them, readings of the wet and dry bulb thermometers were taken, from which both the relative humidity and the weight in grammes of the water vapor in a cubic metre were calculated. The clouds and the state of the atmosphere were also recorded. The potential was always positive, and increased with the elevation. The potential differences for a difference in elevation of one metre varied from 7 volts to 100 volts. No relation appeared between the potential and the humidity. On the whole the potential differences were greater when the sky was clear, though there were some

marked exceptions to this rule. From a series of observations made for me at Princeton by Mr. John Zimmerman of the college, I am led to believe that many of the inconsistencies that appear in any comparison of the potentials with the humidity or with the clearness of the sky are due to the effects of wind. The potentials are generally greater when the wind blows, and this effect of the wind is often more than enough to counterbalance the opposite effect due to a clouded sky, and to give unusually high potentials on cloudy days.

The investigation of the origin and the laws of atmospheric electricity is beset with such difficulties that it is certainly important to obtain the observations upon which our conclusions are to be based at stations at which the controllable conditions shall be as simple and as uniform as possible. My experience at sea, short as it was, convinces me that such observations can be made nowhere to better advantage. A sufficiently large series of them made at several ocean stations might enable us to determine definitely whether the potentials and the humidity are so closely related as Exner maintains, and also to form some accurate notion of the effects due to clouds, to mist, and to wind. The elimination of all occasional disturbances which such a study might render possible would lead to the determination of the value of that most important cosmical constant, the true potential of the earth. It is possible, too, that such a study would reveal some law connecting the observed potentials with the state of the atmosphere, that might be of service in weather prediction.

No place could be better suited for such observations than an isolated lightship. There may be some few lighthouses sufficiently remote from the coast to be suitable stations also, though the fine spray which always fills the air where the sea breaks might prove a serious annoyance, even when in other respects they were most favorably placed. Are not the questions involved of sufficient importance to call for a systematic study of atmospheric electricity by the Weather Bureau? If the proper stations could be secured by the co-operation of the Lighthouse Board, the opportunities offered for useful observations in this field would certainly surpass any that have hitherto been given.

PRINCETON COLLEGE, DEC., 1894.

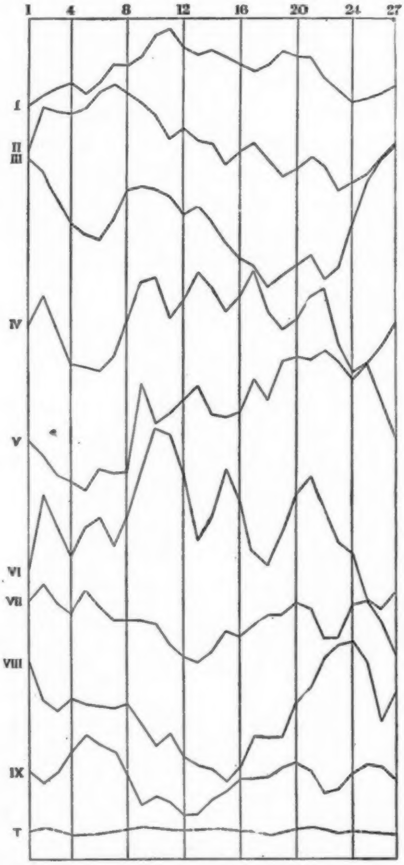
PERIODS IN TEMPERATURE.

PROF. H. A. HAZEN.

IN the October, 1894, number of this JOURNAL, I gave a few curves showing the fluctuations of solar spots on a twenty-seven day interval, and proved that the spots did not have any regular period of twenty-seven days, or any fraction of twenty-seven days. I have recently met a most interesting case substantiating very clearly the claims I set forth in the previous paper, only this time the fluctuations were of temperature in an interval of about twenty-seven days. In studies of this character the seasonal change in temperature becomes a factor which cannot be ignored. In order to eliminate this, in part, we may take the mean temperature for the month and obtain the departure of the temperature for each day from that mean. There is a serious difficulty in this procedure, however, because there are only two months in the year, January and July, in which there is not a marked change in going from the first day to the last. Thus in the Northwestern States the temperature for the first day of each month differs from that on the last day by the following amounts: February, 9°; March, 13°; April, 17°; May, 13°; June, 10°; August, 9°; September, 12°; October, 15°; November, 16°; and December, 14°. Thus it is plain that the departure will be 8° too great on the first day of November, and 8° too small on the last day, and so on. The best way to get rid of this difficulty would be to apply a correction to the observed reading for each day of a month, as determined by a long series of observations, before the departure is taken out.

In any studies for periods, we shall have a very great advantage in taking as many stations as possible, for then accidental variations would be eliminated; but here we meet with another serious difficulty, for we must divide our total effect for all the stations by the number of cases, and this necessarily diminishes the amplitude of the fluctuations. If, however, there is any influence tending to periodicity in a set of observations, this would tend to manifest itself in each case, and the final result would simply eliminate the accidental variations but would show clearly the real periodicity if there be one.

Right here we need to be very careful in our interpretation of results. Suppose we consider that a plus departure of temperature from the mean is a tendency to increased heat, and a minus departure the contrary. If out of one hundred stations



TEMPERATURE FLUCTUATIONS FOR THE WHOLE UNITED STATES.

fifty show $+3°$ and fifty $-3°$, on any one day of a period, every one will admit at once, that there would be absolutely no uniform heating or cooling indicated on that day. If forty-nine stations show $-3°$, fifty show $+3°$, and one shows $+10°$, we would have

a sum of $+13°$, or, dividing by $100 + .13°$, as the influence per station. This, however, would be an extremely erroneous interpretation of the facts in the case, for we really have forty-nine cases showing a cooling and fifty-one cases showing heating, almost exactly the same as before, and we should reason just as before. It seems to me this is an extremely important point and one that should be most carefully considered in all deductions or studies of this kind.

Suppose we combine all the observations for one hundred stations in a general mean for each day of a period, say of about twenty-seven days; with such a very great number of cases we would naturally expect a pretty fair determination of the law in each interval of about twenty-seven days, that is, this law, whatever it is, would run or tend to run through all the intervals. I have already very fully explained this point in the previous paper. One of the surest ways, then, to determine the character of the result would be to project the observations for each interval of twenty-seven days in curves, and then see if there is a thread which is common to each running through different days in all the periods. In other words, while we cannot say "things not equal to the same thing are not equal to each other," we can say "things not equal to or like each other cannot be equal to the same thing."

In a series of summations for fifteen periods, I have taken out the nine in the colder months as better suited for my purpose, and have projected them in the nine curves here shown. These curves show at once that there are no common threads running through them on any particular days of the period and hence there is absolutely no period established in these cases. In fact, curves could not be easily found that differ much more than these. But this is not all; the curve at the bottom shows the true value of the effect, if we assume that there is one, and we see that this effect really amounts to nothing. In fact, a dozen more periods would have reduced the effect to a practical straight line. In this day when there are so many studying questions of this kind and who are liable to fall into grievous error, it seems pertinent to present this analysis of a single phase of the matter.

DEC. 10, 1894.

CURRENT NOTES.

Recent Scientific Balloon Ascents at Munich. — As is generally known, the Munich *Verein für Luftschiffahrt*, which was organized at the end of the year 1889, has been very active in carrying out a series of scientific balloon ascents, the results of which have been published from time to time in the *Jahresbericht* of the *Verein*, in the *Beobachtungen der Meteorologischen Stationen im Koenigreich Bayern* and in the *Zeitschrift für Luftschiffahrt*. The gentlemen who have summarized the data are Drs. Erk, Sohncke, and Finsterwalder, connected with the Meteorological Office in Munich. In view of the fact that the published accounts of the various ascents and of the results gained in them have not been generally accessible, Messrs. Finsterwalder and Sohncke have prepared a summary of the principal results thus far reached. This is printed in the *Meteorologische Zeitschrift* for October, and from this account the following brief notes are taken.

The situation of the city of Munich, close to the northern foot of the Alps, and in the vicinity of some excellent mountain meteorological stations, is a very favorable one for just such an investigation as that undertaken by the *Verein*, owing to the possibility of securing reliable records from surrounding stations during the ascents, and to the fact that the mountain stations furnish data as to the influence of the mountains on the temperature of the atmosphere. It appears from the data obtained in several ascents, by night as well as by day, that in summer, in good weather, the air at the mountain stations is warmer than the free atmosphere during the daytime, but colder during the night, the difference being but a few degrees. In stormy weather the opposite conditions prevail. During the winter the free atmosphere seems to be warmer than that at the mountain stations. It is unlikely that, for the mean of the year, there is any greater difference in the temperatures of the free air and of the air at the mountain stations at the same height, and the conclusion, therefore, is, that the daily and perhaps also the yearly range of temperature is considerably smaller in the free atmosphere than on the mountains.

Observations made in balloons on thunderstorm days furnish some interesting results in regard to the unstable condition of the atmosphere. On June 19, 1889, a day characterized by the occurrence of widespread thunderstorms ("heat" thunderstorms), the temperature observations during the morning hours showed that the atmosphere was already unstable, a fact abundantly proved by the massive cumulus clouds which were generally observed. On July 10, 1889, thunderstorms occurred during the afternoon, and by 11 A. M., the vertical decrease of temperature was such that between the earth's surface and a height of 1,244 meters the atmosphere

was in an unstable condition. July 4, 1892, brought severe thunderstorms six hours after the balloon ascent of that day had shown that the decrease of temperature up to 1,800 meters was at the adiabatic rate, caused by the interchange of upper and lower air masses by means of ascending and descending currents.

Two ascents at night, during anticyclonic conditions, on July 2 and 8, 1893, gave excellent records of the temperatures of the lower air, common during clear, calm nights in summer. On both of these occasions, marked inversions of temperature were noted, the temperature being found to increase from the surface up to about 300 meters, and to decrease above that height. Observations on the decrease of the amount of water vapor in the atmosphere with ascent go to show that the well-known formula of Hann gives, as a rule, results correct within one millimeter. Various interesting observations were made during several ascents in regard to the arrangement of the atmosphere in various well-defined strata, differing in temperature, rate and direction of movement, and in vapor tension. A peculiarly noteworthy case is described as having occurred on Dec. 11, 1890. The balloon rose at 11.18 A. M., and in a few minutes reached the lower limits of a cloud mass, about 100 meters thick, at a height of 1,070 meters. The temperature within the cloud was —6.° C. Above the cloud layer the sun was shining brightly in a deep blue sky, and the clouds below the balloon extended as far as the eye could reach, except in the south, where a magnificent view of the Alps could be obtained. At 1.12 P. M., the balloon began to descend towards the cloud stratum and there were seen to be movements on the upper surface of the clouds, with elevations and depressions amounting to 15 meters in vertical measurement. During the last part of the trip the balloon descended to the upper edge of the cloud stratum, and, for three quarters of an hour, moved along the latter, so that the car was in the clouds, while the balloon itself was in the sunshine. As the balloon drifted along, it followed all the elevations and depressions of the cloud surface, rising or falling as it met with the crests or troughs of the cloud waves.

For convenience of reference the articles hitherto published regarding the Munich balloon voyages are here arranged in chronological order: —

ERK UND FINSTERWALDER: *Die Fahrt des Ballons "Herder" am 10 Juli*, 1889. Jahresbericht des Münchener Vereins für Luftschiffahrt, 1890, p. 19. Also: Zeitschrift für Luftschiffahrt, 1891, p. 18.

FINSTERWALDER: *Adiabatische Zustandsaenderungen in der Atmosphære.* Jahresbericht für 1891, München, 1892. Anlage 9.

ERK: *Die Freie Fahrt des Ballons "München" am* 11 *December*, 1890. Beobachtungen der meteorologischen Stationen im Koenigreich Bayern. Bd. 13. 1891. Also in Jahresbericht für 1891. München, 1892. Anlage 10.

FINSTERWALDER: *Die Freie Fahrt des Ballons "München" am* 4 *Juli*, 1892. Beobachtungen, Bd. 14, 1892. Also in: Jahresbericht für 1892. München, 1893, p. 17.

ERK: *Eine wissenschaftliche Fahrt mit zwei Ballons am* 11 *Juli*, 1892. Ibid.

FINSTERWALDER: *Die wissenschaftliche Ballonfahrt am* 27 *Februar*, 1893. Jahresbericht für 1893. München, 1894, p. 41.

SOHNCKE UND FINSTERWALDER: *Die erste wissenschaftliche Nachtfahrt des Münchener Vereins für Luftschiffahrt. Die zweite wissenschaftliche Nachtfahrt des Münchener Vereins für Luftschiffahrt.* Beobachtungen, Bd. 15, 1893. Also in Jahresbericht für 1893, München, 1894. Pp. 17 and 31.

FINSTERWALDER: *Eine Freie Fahrt am* 12 *Januar*, 1894. Jahresbericht für 1893, München, 1894, p. 47.

Influence of Telephone Wires on Atmospheric Electricity.—The German Department of Telegraphs has been carrying on an investigation with a view to determining what effect the telephone wires in cities have on atmospheric electricity, *i. e.*, whether the danger from lightning is increased or decreased by the presence of the miles of wires now necessary in cities in which the telephone is extensively used. According to *Das Wetter* (November, 1894, page 264), the result of the investigation has been to show that the wires tend to weaken the severity of thunderstorms and to decrease the danger from lightning. From information received from 340 cities with telephone wires, and from 560 without them, it appears that the danger of damage by lightning in the two classes is as 1 to 4.6. Although, as is the common experience, the danger from lightning is greater in the country than in cities, and it is therefore to be expected that the towns without telephones, which are generally country villages, should show a greater percentage of damage by lightning than large cities, yet the difference cannot be more than fifty per cent; while in the figures just given it is seen that places without telephones are almost five times more liable to damage by lightning than the places with telephones. A further interesting point is this: that in the former class, *i. e.*, places without telephones, five lightning strokes on the average occur during every hour of thunderstorm activity, while in cities with telephones only three occur. It appears, therefore, that the presence of many wires in our cities is a protection against damage by lightning, and does not increase the damage as has been often supposed.

Meeting in Memory of the late Prof. von Helmholtz. — It should be a source of singular pride to meteorologists that one of their brightest lights should have had the honor to deliver the memorial address on Helmholtz. On Dec. 14, 1894, royalty and learning united to pay homage to the memory of the great departed. On that day the memorial services to its late president were held by the Berlin Physical Society, and with it were joined fourteen other prominent associations, among them the German Meteorological Society and the Society for the Promotion of Aeronautics. The meeting was held in the Berlin *Sing Academie;* and the assemblage there gathered together was a most impressive one, the gentlemen being in full dress, the ladies in black. It was attended by the Emperor, the Empress, and the Empress Frederick; by high state dignitaries, and by brilliant lights in the German scientific world. Rising from the lower platform, and adorned with laurels and palms, was a tall pedestal, surmounted by a colossal bust of the

great von Helmholtz. Arranged in rising tiers, back of the lower platform, sat the chorus; back and around them were arranged the representatives of the various University student societies, in full uniform, with their flags and emblems, making a most effective background. At the appearance of the Emperor, at 12 o'clock, noon, the exercises at once began with a hymn by the chorus. This was followed by the scholarly oration delivered by the President of the Berlin Physical Society, Prof. W. von Bezold. The speaker briefly, and in well-chosen words, recounted the many and varied discoveries achieved by von Helmholtz in his long and industrious life. He spoke more particularly, however, of the lovable personal qualities of the man, being specially qualified to do this by reason of his intimate relations with him. The address was followed by a masterly rendition of Schumann's "Abendlied," by the famous Berlin violinist, Prof. Joachim. The exercises closed with another hymn by the chorus. L. A. B.

Royal Meteorological Society. — The monthly meeting of this Society was held on Wednesday evening, Dec. 19, at the Institution of Civil Engineers, Great George Street, Westminster; Mr. R. Inwards, F. R. A. S., President in the chair.

Twenty-six new Fellows were elected.

Mr. H. Southall, F. R. Met. Soc., read a paper on "Floods in the West Midlands," in which he gave an interesting account of the great floods which have occurred in the rivers Severn, Wye, Usk, and Avon. He has collected a valuable record of the floods on the Wye at Ross, which he arranges in three classes, viz.: (1) primary or highest of all, those of 14 ft. 6 in. and above; (2) secondary, those with a height of 12 to 14½ ft; and (3) tertiary, those with a height of 10 to 12 ft. The dates of the floods above 14 ft. 6 in. are as follows: 1770, Nov. 16 and 18; 1795, Feb. 11 and 12; 1809, Jan. 27; 1824, Nov. 24; 1831, Feb. 10; 1852, Feb. 8 and Nov. 12. The height of the recent flood on Nov. 15, 1894 was 14 ft. 3 in. which was higher than any flood since November, 1852. The flood on the Avon at Bath on Nov. 15, 1894, is believed to have been the highest on record.

Mr. R. H. Scott, F. R. S., gave an account of the proceedings of the International Meteorological Committee at Upsala, in August last, with special reference to their recommendations on the classification of clouds and the issue of a Cloud Atlas.

A paper by Mr. S. C. Knott was also read, giving the results of meteorological observations made at Mojanga, Madagascar, during 1892 to 1894.

Scientific Balloon Ascents in Berlin in August. — The investigation of the upper air by means of balloons has been energetically continued in Berlin. On Aug. 1 an ascent was made by Dr. Berson, starting at 12.30 P. M., in the balloon "Falke." The sky was covered with low-lying stratocumulus clouds, whose lower edges were between 1,050 and 1,100 meters above the surface of the earth. At 1,350 meters the balloon left the clouds and came into bright sunshine, which produced a rapid rise of the black bulb thermometer from 66° Fahr. to 129° Fahr., and a rise of the air tem-

perature from 45.32° Fahr. to 55.22° Fahr. The height reached was 1,700 meters (5,577 ft.), where the temperature was about 60° Fahr.

On Aug. 4, at 6.45 A. M., the balloon "Phönix" was sent up with Lieut. Gross and Dr. Berson, the weather being unsettled and somewhat cloudy, and the wind west-southwest. By dint of active telegraphic correspondence it had been arranged that a balloon ascent should be made from St. Petersburg at nearly the same time. The St. Petersburg balloon went up during the night, some hours previously. About 9 A. M. the voyagers in the "Phönix" noticed the growth of cumulus clouds, which, by noon, became very massive and presented distinctly the appearance of a thunderstorm. The region traversed by the balloon was visited by numerous thunderstorms during this day. At a height of 12,300 ft. a thin veil-like snow-cloud was passed through, above which there was clear blue sky. The greatest height reached was 12,800 ft. and the lowest temperature 23° Fahr. The relative humidity was least at 5.30 P. M. at a height of about 5,400 ft., when it was 22%.

Aug. 9, in response to an urgent request from St. Petersburg, an ascent was made from Berlin in very cloudy weather. Profs. Börnstein and Baschin and Dr. Berson went up at 7.30 A. M. The maximum altitude was 11,500 ft.; the mean velocity of progression was somewhat over 48 ft. per second. The temperature decreased up to a height of 10,800 ft., and above that increased slowly.

The foregoing facts are taken from a paper by Dr. Berson in the November number of the *Zeitschrift für Luftschiffahrt.*

Annual Report of the Saxon Meteorological Institute.—Prof. Dr. Paul Schreiber's Annual "Bericht ueber die Thaetigkeit im Koenigl. saechsischen meteorologischen Institut auf das Jahr 1893," published as Part II. Vol. II. of the *Jahrbuch* of the Saxon Meteorological Institute, is at hand. During the year 1893 the Director completed a work on the Climatography of Saxony, which appears as Part I. of Vol. VIII. of the *Forschungen zur Deutschen Landes- und Voelkerkunde*, published by Engelhorn in Stuttgart. He also prepared a paper on the Artificial Production of Rain in the United States, and two papers for the Meteorological Congress at Chicago, one of which, on the Thunderstorms of Saxony, has already been reviewed in this JOURNAL, Vol. XI., pages 24, 25. Dr. Schreiber has further spent a large share of his time in investigations concerning the climate of Saxony.

The verification of forecasts, made at the Central Station in Chemnitz by Dr. H. Lindemann, but not published, shows that 73 % of the temperature and 61 % of the precipitation forecasts were wholly verified, and 17 % and 33 % respectively were partly verified. Regular meteorological observations were made at 167 stations, of which 1 was of the first class, 20 of the second, 7 of the third, and 139 of the fourth. One hundred and forty-eight stations reported thunderstorms; 122 observers reported depth of snow on the ground during the winter of 1893–94, and 34 reported phenological data.

Annual Report of the Iowa Weather and Crop Service.—Mr. J. R. Sage, Director of the Iowa Weather Service, has recently issued the fourth Annual

Report of that service. About twelve hundred voluntary observers and crop correspondents, representing every county in the State, furnished data for this Report. The crop bulletin was issued during twenty-five weeks, the average issue being 1,800. The *Monthly Review* reached an edition of 25,800 copies. The mean temperature for the year was 1.3° below the normal; the highest temperature reported was 102°, and the lowest, — 28°. The precipitation was about 8 inches below the normal.

Hodgkins Fund Prizes. — The following circular has been issued by the Smithsonian Institution, Washington D. C.: —

SMITHSONIAN INSTITUTION.

HODGKINS PRIZES.

WASHINGTON, Jan. 10, 1895.

The time for the reception of treatises or essays offered in competition for the Hodgkins Fund Prizes of $10,000, of $2,000, and of $1,000 respectively, closed on the 31st of December, 1894, and all papers so offered, are now in the hands of the Committee of Award.

In view of the very large number of competitors, of the delay which will be necessarily caused by the intended careful examination, and of the further time which may be required to consult a European Advisory Committee, if one be appointed, it is announced that authors are now at liberty to publish these treatises or essays without prejudice to their interest as competitors.

S. P. LANGLEY,
Secretary of the Smithsonian Institution.

Death of Father Francesco Denza. — Father Francesco Denza died in Rome, on Dec. 14, 1894. He was, at the time of his death, Director of the Vatican Observatory, Director-General of the Italian Meteorological Society and Director of the Observatory at Moncalieri.

Very extensive investigations were undertaken by Father Denza, as Director of the Vatican Observatory, in meteorology, terrestrial magnetism, geodynamics, and astronomy. The fourth volume of the *Pubblicazioni* of that institution has recently been issued. It was due to the untiring energy of Father Denza that the *Corrispondenza Meteorologica Italiana* was established in connection with the Alpine Clubs, and the results of the observations at a large number of stations in the Alps and Apennines have been regularly published in the organ of the Italian Meteorological Society.

Hellmann's "Neudrucke." — A few copies of the interesting reprints of old and rare books (reviewed in this JOURNAL, Vol. X., pp. 198–200, Vol. XI., pp. 119,120) may be obtained of A. L. Rotch, Blue Hill Observatory, Readville, Mass.. at the prices at which they were published in Berlin, viz.: —

No. 1. Reynmann. Wetterbuechlein. 6 marks = $1.50.

No. 2. Pascal. Récit de la Grande Expérience de l'Equilibre des Liqueurs. 3 marks = 75 cents.

No. 3. Howard. On the Modifications of Clouds. 3 marks = 75 cents.

Meeting of the International Meteorological Committee. — *Nature*, which had reprinted this report from the December JOURNAL, is informed that the following statement is misleading: "A proposition of the Russian Admiral Makaroff, on the necessity of an international convention to arrange for the discussion of the data contained in ships' logs, was not approved."

As the writer now understands the matter, although Admiral Makaroff had sent printed statements of his scheme to some members of the committee and it had been included in the secretary's list of questions, yet it was not considered a subject for discussion, and that, therefore, no official opinion was expressed in regard to it.

Weather Bureau Notes. — Mr. Chaffee, Local Forecast Official at Montgomery, Ala., and Mr. Blythe, Local Forecast Official at Cairo, have been ordered to Washington for two months' practice work in forecasting.

The Secretary of Agriculture has increased the salaries of six of the local forecasters, the selection being made on the grounds of efficiency of the forcaster and the importance of the interests served. New York, Philadelphia, Boston, Galveston, St. Louis, and New Orleans are the stations thus benefited.

CORRESPONDENCE.

THE TERMS CYCLONE AND TORNADO.

Editor of the American Meteorological Journal:

Formerly people in this country used to be correct in calling a destructive local whirlwind a *tornado*, but lately they have taken to the untenable position of calling this peculiar form of storm a *cyclone*, probably because this word sounds more learned and more impressive. We all know the woful and wilful ignorance of newspaper reporters and others who retail current accounts of tornadic visitations throughout the States in the daily papers, and starting from here this unscientific and untechnical designation of a local whirlwind as a *cyclone* has actually passed over to the press in Great Britain and France, which should, above all, be free from such errors.

The observers in a district swept over by a tornado in the southeast quadrant of a cyclone insist on confounding the funnel cloud with the main cyclone, and set down the devastation wrought by the tornado to the latter. Moreover, these observers make the most absurd blunders regarding the tracks of two or more neighboring tornadoes, and often describe the path as very zigzaggy, and as extending in a northwesterly, southwesterly, or westerly direction. The same kind of a mistake in terminology would be made by an observer at sea who would confound the closely allied phenomenon of a waterspout with the general rotating cyclone.

A fully developed waterspout, passing over buildings in the Bermudas, for instance, would work as much devastation to the structures there as would a similarly developed tornado in the States in passing over thickly-populated settlements.

Our widely-renowned Weather Bureau has attained a proud position through its forecasts of cyclones and thunderstorms. Now let it go a step further. Let it entirely drop the cumbersome terms, *Highs* and *Lows*, *High Barometer* and *Low Barometer*, *High Barometric Area* and *Low Barometric Area*, and take up the more technical expressions *cyclones* and *anticyclones*. The Chief of the Weather Bureau has lately instituted a particular signal for heralding the approach of cyclones from the West Indies, which is called the "hurricane signal." I thoroughly believe in a special signal for the tropical hurricane, but I would have it designated as the tropical *cyclone* signal. I would go still further, and devise specific signals for the *thunderstorm* and *tornado*. For foretelling the probable occurrence of a thunderstorm I would hoist a square white flag with a large T in the centre. This letter would be as conspicuous as the number on the sail of a pilot boat, which, as is well known, can be seen at great distances at sea. For indicating the probable occurrence of a tornado, that is, the maximum development of the thunderstorm, I would hoist two T storm signals, one above the other, as in the case of the new "hurricane signal."

H. P. Curtis.

Boston, Mass., Dec. 30, 1894.

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THE TYPHOONS OF THE YEAR 1893.

S. Chevalier. *The Typhoons of the Year* 1893. Second Annual Report of the Shanghai Meteorological Society for the Year 1893. 8vo. Zi-Ka-Wei, 1894. Pp. 97, Pls. XVIII.

The Shanghai Meteorological Society promises to be an active body, if we may judge from its first two annual reports, the second of which has recently come to hand. This report, like the previous one, has been prepared by the Rev. Father S. Chevalier, S. J., the Director of the Zi-Ka-Wei Observatory and the President of the Society. It deals with the typhoons of the year 1893.

In his pamphlet Father Chevalier takes up each typhoon separately, tracing it from its first reported appearance until its disappearance, and following out the changes of pressure, wind, and weather. There are numerous plates giving barometic curves, typhoon tracks and isobaric charts, and tables of data as recorded at various meteorological stations and on board vessels. At the conclusion of the report Father Chevalier considers the tracks of typhoons, and in so doing finds it necessary to divide these dis-

turbances according to the time of their occurrence, *i. e.*, the typhoons of May, June, July, etc., and also according to the countries which they visit, viz., Japan, China, or Cochin-China typhoons. As a result of his investigations the author concludes as follows: "At the beginning of the typhoon season, before the temperature has attained a high degree in extra-tropical latitudes, and at the end of the season, when the hot period has passed, the typhoons do not ascend the China coast above the tropics. In Japan the typhoons reach extra-tropical latitudes from the beginning of the typhoon season and keep on travelling towards these latitudes up to November, a long time after they have ceased reaching the same latitudes on the China coast. . . . As far as I am able to understand the matter, two causes are at the bottom of these variations in the trajectories of typhoons on the China coast, and the differences . . . between the Chinese and Japanese systems of typhoons. The first is their place of origin, which is somewhat up towards the north, with the rising of the temperature in northern hemisphere, and falling down to the equator with the fall of the temperature in northern hemisphere. The second cause is the distribution of the barometic pressures on Eastern Asia, and its variations with the periods of the year." The main features of the distribution of pressure are, during the winter monsoon period, a centre of very high pressure in Siberia, and of low pressure between Japan and Siberia. There is also a centre of high pressure about latitude 30° N., south of the centre of low pressure. Between the equator and the tropics there is a gradient perpendicular to the equator. During the summer monsoon period the main centre of high pressure is over the Pacific about latitude 40° N. and 150° E., the low pressure centre being over India, extending to China and Siberia. A secondary high area lies north of Behring Strait, and a secondary low over Eastern Siberia. The distribution of pressure thus outlined reverses with the seasons, and is the cause of the monsoons, and also, Father Chevalier believes, of the variations in the trajectories of the China typhoons and of the differences between the China and Japan typhoons.

Father Chevalier's report is a useful one for students of cyclonic tracks and phenomena, and his conclusions, based on a thorough knowledge of the facts, are well worth careful consideration.

BAROMETERS AND THE MEASUREMENT OF ATMOSPHERIC PRESSURE.

C. F. MARVIN. *Barometers and the Measurement of Atmospheric Pressure. A pamphlet of Information respecting the Theory and Construction of Barometers in General, with Summary of Instructions for the Care and Use of the Standard Weather Bureau Instruments.* U. S. Department of Agriculture, Weather Bureau, Circular F, Instrument Room. 4to, Washington, D. C., 1894. Pp. 74. Figs. 24.

This is a useful report on barometers, designed especially for the instruction and guidance of observers of the United States Weather Bureau, but which any student of meteorology will find valuable in studying the theory,

construction, and use of these instruments. The pamphlet is a very complete one. In it Prof. Marvin considers the various kinds of barometers, describing their construction minutely; the errors of barometers and the corrections to be applied, and barographs. The General Instructions for the care and use of barometers and barographs are very clear and full, and at the end are given tables showing the correction of mercurial barometers for temperature; the reduction of the barometer to standard gravity, and the determination of heights by the barometer. The report is illustrated with twenty-four figures. It appears as Circular F of the Instrument Room of the Weather Bureau.

TITLES OF RECENT PUBLICATIONS.

FURNISHED BY MR. OLIVER L. FASSIG, LIBRARIAN, U. S. WEATHER BUREAU. WASHINGTON, D. C.

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